Second review of:

Exploring the role of snow metamorphism on the isotopic composition of the surface snow at EastGRIP

By Harris Stuart et al.

General comments

While again, this approach to link snow metamorphism to isotopic change is a great idea, I am skeptical about the way it is implemented. Again, my reservations are based on the fact that the samples used for SSA measurements (1 cm surface layer) and those used for isotopic measurements (2.5 cm surface layer) are not the same. In this revised version, the Authors do take care to some extend of surface processes by selecting low wind events where snow drift and surface perturbations may have been absent or minimal. However, even the low wind events feature surfaced perturbations. Event E10 was a 1 cm thick snowfall, and event E11 was a thin fog deposit. The surface layer sampled for SSA was then a different layer that the lower 1.5 cm, included in the isotopic sample.

Furthermore, the protocol is not explained clearly. It is really very confusing and figuring out what was done is a headache, at least for me. Some crucial elements are given too late in Results rather than in Methods, Table A1 is incomplete and therefore not very useful. Perhaps the isotopic data should be analyzed differently, by considering a surface layer with rapid changes and a lower layer with low changes. This would however add some uncertainty. I am therefore not sure what to recommend, because it is quite clear to me that the sampling protocol is just not adequate and inevitably skews the results, possibly irremediably.

This is unfortunate, because the topic has great potential. Time series of surface snow SSA and isotopic compositions are highly valuable, but correlating them requires both samples to be identical. In that line, I do not know what to make of the EOFs of Figure 3. I may have missed something, but were all the SSA and isotopic data used to obtain them? Surely some serious filtering would have been mandatory here.

Anyway, I am really undecided here. On the one hand, the experimental data does have some value as those time series are unique and deserve publication in some form. On the other hand, the interpretation and the correlations seem extremely weak to me. The Authors failed to sufficiently stress the enormous caveats in their protocol, and their conclusions have a very weak basis that may in fact be misleading. Perhaps the Authors could stress their caveats and tone down their interpretation to present their data in what I feel would be a more honest and humble manner. I am just trying to help here, by the way.

Specific comments

Line 42-43. Decrease in SSA is explained by Ostwald ripening only under almost perfect isothermal conditions, which is rarely the case here. How about "Decrease in SSA in dry snow is predominantly the result of water vapor transfer among grains, with smaller grains feeding the growth of larger grains. Ventilation by wind can accelerate SSA decrease by enhancing water vapor transfer rates.". This statement also applies to Ostwald ripening but is much more general. Ostwald ripening is very specific.

Line 50-51. Stating that "Exponential models are documented to produce the best fit to in-situ SSA decay data". is probably exaggerated, as equations given in (Legagneux et al. 2004) and also used in the models of (Flanner and Zender 2006) probably are more accurate. Perhaps say something like "Are the most convenient to account for temperature effects under various wind conditions".

Line 52. Should be (Legagneux et al. 2004).

Line 107. "The e-folding depth of 1310nm radiation in snow of 200 kgm−3 is approximately 1 cm". This is true for a given value of SSA. Please specify which SSA value and perhaps give details regarding the impact of the SSA value on the e-folding depth. Calculations can be done done e.g. using https://snow.univ-grenoble-alpes.fr/snowtartes/

Line 126. The SSA of surface hoar is usually moderate to low, so that surface hoar formation can often lead to SSA decrease.

Section 2.5.1. It is not clear which SSA data were finally filtered out. Many confusing elements are given but no clear criterion is given in the end. So, were the events with wind speed <6 m/s kept and those >7 removed? Then there are moderate wind events. What was done with the SSA data in that case? This must be written simply and clearly. Some elements of section 3.2.1 should probably be placed in this methods sections, as they are not results.

Lines 194-200. Were all the SSA data used for the EOF? But some are unreliable, I think. This all needs to be clarified.

Line 147. Cabanes 2002 and Cabanes 2003 should be swapped.

Equation (4). Is that for isothermal or temperature gradient conditions?

Line 166. Not sure what signal attenuation means here.

Line 187-188. "Throughout the season $\delta^{18}O$ follows a gradual increasing trend from May to August following increasing temperatures." This may be overly simplified. There are drops in $\delta^{18}O$, especially at the end of all seasons, while temperature does not drop. This is mentioned later, but nevertheless this statement is not warranted.

Line 221. It thus appears that even E11 is characterized by a thin fog deposit. Therefore, the SSA decay rate may pertain to the top few mm. Comparing its evolution to the isotopic evolution of the 2.5 cm thick isotope snow samples may then be meaningless.

Section 3.2.2 Which events are used to construct this model? This should be clearly mentioned and added to Table A1. Line 303 mentions 6 events were used, but we need to know that now.

Line 237. I am not sure the value 22 m^2/kg can be called a decay constant. A decay constant is expected to have s^{-1} in its units, I would think.

Line 314. Please specify the temperature range.

Section 4.1. Please note that both T07 and FL06 ignore wind speed as a variable. Those studies are all based on data obtained under no wind or low wind speeds. Since wind speed accelerates SSA decay, as first noted by (Cabanes et al. 2002), it is not surprising that both T07 and FL06 underestimate the decay rate observed by the Authors under non-negligible winds. This may be explicitly mentioned.

Lines 333-338. I do not understand the difference between mechanisms 1 and 2. Large grains grow at the expense of small grains by water vapor diffusion, so I just do not see the difference with diffusion of interstitial vapor. Then the following discussion may need to be significantly rewritten. It may be somewhat more sensible to consider the temperature gradient. Elevated gradients drive fluxes throughout layers while low gradients mostly involve short distance vapor transfers less likely to result in isotopic changes. Wind pumping is more likely to result in the largest changes since this is where the exchanges of vapor with the atmosphere will be greatest. In any case, looking at Figure 2, my guess is that wind pumping will be important in all cases so that this will always be the predominant process. I therefore have the feeling that the Authors are on the wrong track and that their classification of events is inadequate. I would be more tempted to consider other criteria, such as perturbations of the snow surface, where the sign of LE would come in.

Line 340. Reference to Ebner is incorrect. The discussion paper is mentioned.

Line 357-359. "We conclude that SSA of the surface snow is strongly influenced by surface-subsurface TG while the changes in isotopic composition are likely to be influenced by other factors such as the magnitude of vapour-snow isotopic disequilibrium during sublimation". The Authors may be right here. But the picture would probably be clearer if the same samples had been used to measure isotopes and SSA. The different sampling depths really skews the results and make any interpretation uncertain.

Line 383-385. Please consider that given the windy context, wind pumping is a much more efficient process that temperature gradient-induced diffusion to produce exchanges of water vapor and isotope fractionation in the surface snow.

Table A1 should have an extra column indicating wind conditions.

References cited

Cabanes, A., L. Legagneux, and F. Domine, 2002: Evolution of the specific surface area and of crystal morphology of Arctic fresh snow during the ALERT 2000 campaign. *Atmos. Environ.*, **36,** 2767-2777.

Flanner, M. G., and C. S. Zender, 2006: Linking snowpack microphysics and albedo evolution. *J. Geophys. Res.*, **111,** D12208.

Legagneux, L., A. S. Taillandier, and F. Domine, 2004: Grain growth theories and the isothermal evolution of the specific surface area of snow. *J. Appl. Phys.*, **95,** 6175-6184.